Responses of tidal wetlands to invasive cordgrass, sea level rise, and sediment supply

Debra Ayres

Final Selection Panel Review

Proposal Title

#0047: Responses of tidal wetlands to invasive cordgrass, sea level rise, and sediment supply

Funding:

Fund with future funds
Amount: **$500,000**

The final Selection Panel agreed with its original recommendation on the merits of this proposal. Due to the recent reduction in funds available for the Science Program's 2004 PSP, the Selection Panel has been forced to place this proposal in the Fund with Future Funds category. This decision was based solely on the current programmatic priorities of CALFED and the current level of available funds for purposes of supporting research efforts of this nature. This decision was not a reflection of the technical merit of this proposal.

Public Comments

No public comments were received for this proposal.

Initial Selection Panel Review

Proposal Title

#0047: Responses of tidal wetlands to invasive cordgrass, sea level rise, and sediment supply

Funding:

Fund in part
Amount: **$500,000**

Initial Selection Panel (Primary) Review

Topic Areas

- Environmental Influences On Key Species And Ecosystems
- Implications Of Future Change On Regional Hydrology, Water Operations, And Environmental Processes

Please describe the relevance and strategic importance of this proposal in the context of this PSP. How does the proposal address the topic areas identified above? What are the broader CALFED Goals this proposal may meet that are not accounted for in these specific topic areas?

This is a collaborative proposal to parameterize and apply a model that predicts marsh elevation as a function of cordgrass growth rate, sediment supply and sea-level rise. This model has effectively been used by one of the PIs on the east coast. The model will be parameterized for the native California cordgrass species, the invasive cordgrass species, and the native-invasive hybrid, which is more invasive and more productive than either parent species. The strategic importance of this research lies in two areas: (1) The proposal addresses the interaction of environmental changes (sea level rise and sediment supply) and a highly invasive species that has the potential to profoundly change the marsh and shallow water environment of the Bay. These changes would impact many other species dependent on marsh and shallow water habitats. Changes in the ratio of vegetated marsh to open

water would impact estuarine food webs as well as water residence times. The incorporation of both immediate concerns (a highly invasive species) with long-term change (sea level rise) in a modeling framework should provide Calfed with a powerful tool. The proposal addresses environmental influences (sea level rise and sediment supply) on key species (invasive and native cordgrass) and ecosystems (the host of species that would be impacted by a change in marsh conditions). It also addresses the implications of future change (sea level rise, changing sediment supply) on key environmental processes (cordgrass invasion, sedimentation leading to changes in marsh elevation, primary productivity). (2) The model should prove extremely useful to the design and success of salt marsh restoration projects and enable practitioners to evaluate the vulnerability of hybrid-invaded marshes to geomorphological changes with implications for many aspects of the ecosystem. Research that will enhances the likelihood of success of restoration projects is a broader Calfed goal not accounted for in the topic areas.

The budgets of proposals submitted in response to this PSP are larger, on average, than those submitted to CALFED in previous years. The Science Program is committed to getting as much science per dollar as is reasonably possible. With this commitment in mind, can the proposed budget be streamlined? If so, please recommend and clearly justify a new budget total in the space provided.

The proposed budget is reasonable. However, if the project has to be cut in any way, the biogeochemical analyses seem to be one piece that could be reduced without completely destroying the value of the project. From the equations presented in the proposal, it did not seem as though all the biogeochemical measures (pore water biogeochemistry) were absolutely essential for model success. That aspect of the proposed research is currently budgeted at $105,512. It seems fairly awkward and expensive to be sending these water samples across the country for what are fairly standard analyses. Surely there are labs in California that could do these analyses.

Evaluation Summary And Rating.

Provide a brief explanation of your summary rating and any additional comments you feel are pertinent.

This is a proposal with a high likelihood of success in an area of strategic importance for Calfed.

Selection Panel (Discussion) Review

fund this amount: $**500,000**
note:
fund in part

The panel was interested in the project's focus on the mechanisms by which a dominant invasive species and climate change affect sediment dynamics in salt marshes and mud-flats. These processes influence the rate and extent of salt marsh formation in the San Francisco Estuary. The forward-thinking, big-picture view promised by the proposed modelling were seen as strengths of this proposal. Also, the panel appreciated the strengths of the interdisciplinary research team and felt they were likely to produce important results.

The panel was concerned that this project does not address the highest-priority species in the CBDA program. Specifically, the products of this research will not necessarily help resolve conflicts over resource allocation among CBDA stakeholders. However, invasion by cord grass hybrids is ongoing and the proposed research timely. It will likely provide important insights into ecosystem processes in CALFED's Bay-Delta region, and may have implications for the quality and quantity of habitat in the bay for fish and birds, including at-risk species.

The panel recommended funding at a reduced level (ca. $500K). The panel recommended reduction in the funds allocated to biogeochemical analyses.

Collaboration Panel Review

Proposal Title

#0047: Responses of tidal wetlands to invasive cordgrass, sea level rise, and sediment supply

Final Panel Rating
above average

Collaboration Panel (Primary) Review

Collaboration:

Will the results of the collaborative effort be greater than the sum of its parts? Is it clear why the subprojects are part of a larger collaborative proposal rather than several independent smaller ones?

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above average
There are two parts to this proposal - the first involves
collecting and analyzing field-based data that will result in
parameter estimates. The second part involves using these
estimates to refine a model tha was originally developed for
use on the East Coast. The resulting collaborative effort will
be greater than the sum of the parts. The collaboration
involves two institutions.
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Interdependence And Integration:

Does the proposal have an example that clearly articulates the conceptual model of each subproject and how they link together as a whole? Are the boundaries of the study plans focused and cohesive, yet well delineated? Is there a plan for potential differences in the stages of subproject completion times? Are there clear plans for analyses and interpretations which seek to identify and quantify relationships among the data collected in various subprojects rather than separate analyses for each subproject?

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above average
The proposal has one conceptual model that links both
subprojects. There is one study plan with the two parts of the
projects occurring sequentially. There are also two separate
field sites. There is not a plan for potential differences in
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the stages of the subproject completion time as the tasks are designed to occur sequentially.

Project Management:

Is it clear who will be performing management tasks and administration of the project? Are there resources set aside for project management and time given for investigators to collaborate? Is there a process for making decisions during the course of the project? Are there acknowledgments of potential barriers to collaboration and explanations of how team members will overcome barriers particular to their institutions?

adequate
UCD will administer the project. In the personnel section, it states that Dr. Ayres from UCD will "coordinate grant/research activities between UCD and USC teams." From the budget and task list, there is no clear process outlined for collaboration and joint decision-making.

Team Composition:

Does the lead principal investigator have successful management history and experience leading collaborative teams? Is it clear that all key personnel are committed to making significant contributions to the project? Do team members have complementary skills?

Team members have complementary skills. There is no evidence that the lead PI has a history of leading collaborative efforts.

Communication Of Results:

Is there a clear plan for comprehensive and cohesive reporting of project progress to the CALFED community?

adequate
The proposal indicates that CALFED progress reports and scientific papers will be written. In addition, the proposal states an intent to present results at the CALFED Science Conference. There is no comprehensive plan, however, for outreach and dissemination of results.

Additional Comments:

Collaboration Panel (Discussion) Review

Primary reviewer felt the proposal was overall adequate since the two institutions involved described a conceptual model that links both projects and builds on information generated over te years from east coast ecosystems. There was no plan for differences in stages of tasks, but felt it may not be needed. The negatives in the proposal were that there was no clear process for budget and decision-making; applicants lists CCC as partner but does not identify what they will do. Also, the proposal did not incorporate project management components.

The secondary reviewer gave an overall rating of superior because he also viewed the collaboration based on knowledge and location of institutions (which were assets of the lead investigators). Noted that some inefficiencies in communication could arise because of distance between collaborators.

The final rating given by consensus was above average, with the comment that the proposal needs to better develop descriptions of management tasks and the associated budget.

Technical Synthesis Panel Review

Proposal Title

#0047: Responses of tidal wetlands to invasive cordgrass, sea level rise, and sediment supply

Final Panel Rating
superior

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The investigators propose to examine tidal marsh stability by examining the interactions among sea level, land elevation, primary production, and sediment accretion. They argue that a model developed for the east Coast merits examination for the tidal marsh ecosystems in the San Francisco estuary, for which such a model has not been developed. They describe the model well and explain how the parameters would be evaluated for the San Francisco Bay. The model is likely to prove useful in evaluating marsh stability with respect to cordgrass invasion. The project will be a collaboration between UC Davis and University of South Carolina (where the marsh model was developed) and includes collection and analysis of physical and primary productivity data at two field sites: a northcentral Bay marsh and a southcentral Bay marsh. The field data will be used to develop parameter estimates for a mathematical model that has been successfully used to examine the interaction of primary productivity, sediment supply, and sea level rise on geomorphology in a South Carolina tidal salt marsh. The proposal is very strong on the conceptual and theoretical background for the work, which is replicating methods successfully applied on the Atlantic coast. Three reviewers gave overall ratings of VERY GOOD, EXCELLENT, EXCELLENT. All agreed that the issues are important and timely and that the investigators are well qualified. The reviewer

rating the proposal as "very good" was disappointed that only two study sites were chosen, given that the range of parameter values for the Bay region is sought. This reviewer also questions some of the assumptions behind transferring the model to the Pacific coast (e.g. "The PIs need to keep in mind that spartina in the SE works differently in at least a few significant ways from that in the West"), although these assumptions and characteristics will presumably be evaluated in the process of testing the model in SF Bay. This reviewer suggests a pilot project, with a much smaller budget; followed by a project with many more sample sites. The other two reviews were informed and highly supportive in all aspects. For example "The project goals are focused and clearly stated: to understand how hybrid cordgrass invasion, sediment supply, and sea level rise affect long-term sustainability of salt marshes in the San Francisco estuary. These goals are timely given the rapid spread of Spartina alterniflora and its hybrids with Spartina foliosa in the Bay and the threat of sea level rise to coastal wetlands. The goals are directly relevant to CALFED priorities of managing and restoring habitats and are of current interest in the scientific literature" and "The results of this project will be extremely useful in selecting future restoration projects, and adaptively managing existing projects".

Additional Comments:

The investigators propose to examine tidal marsh stability by examining the interactions among sea level, land elevation, primary production, and sediment accretion. They argue that a model developed for the east Coast merits examination for the tidal marsh ecosystems in the San Francisco estuary, for which such a model has not been developed. They describe the model well and explain how the parameters would be evaluated for the San Francisco Bay. The model is likely to prove useful in evaluating marsh stability with respect to cordgrass invasion. The project will be a collaboration between UC Davis and University of South Carolina (where the marsh model was developed) and includes collection and analysis of physical and primary productivity data at two field sites: a

northcentral Bay marsh and a southcentral Bay marsh. The field data will be used to develop parameter estimates for a mathematical model that has been successfully used to examine the interaction of primary productivity, sediment supply, and sea level rise on geomorphology in a South Carolina tidal salt marsh. The proposal is very strong on the conceptual and theoretical background for the work, which is replicating methods successfully applied on the Atlantic coast. Three reviewers gave overall ratings of VERY GOOD, EXCELLENT, EXCELLENT. All agreed that the issues are important and timely and that the investigators are well qualified. The reviewer rating the proposal as "very good" was disappointed that only two study sites were chosen, given that the range of parameter values for the Bay region is sought. This reviewer also questions some of the assumptions behind transferring the model to the Pacific coast (e.g. "The PIs need to keep in mind that spartina in the SE works differently in at least a few significant ways from that in the West"), although these assumptions and characteristics will presumably be evaluated in the process of testing the model in SF Bay. This reviewer suggests a pilot project, with a much smaller budget; followed by a project with many more sample sites. The other two reviews were informed and highly supportive in all aspects. For example "The project goals are focused and clearly stated: to understand how hybrid cordgrass invasion, sediment supply, and sea level rise affect long-term sustainability of salt marshes in the San Francisco estuary. These goals are timely given the rapid spread of Spartina alterniflora and its hybrids with Spartina foliosa in the Bay and the threat of sea level rise to coastal wetlands. The goals are directly relevant to CALFED priorities of managing and restoring habitats and are of current interest in the scientific literature" and "The results of this project will be extremely useful in selecting future restoration projects, and adaptively managing existing projects".

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The proposal addresses a very timely and important topic, the spread of invasive cordgrass through the San Francisco Estuary. The applicants propose to model the impact of sea-level rise and sediment supply on the spread of cordgrass. The applicants plan to validate and parameterize their model (developed for east coast marshes) through intensive study on two study areas. The panel supported this approach. External reviewers questioned whether the models could be scaled-up to account for the spread of cordgrass at the landscape level. The applicants are aware of the difficulties in applying model results across scales and specifically discuss how the results are likely to scale. The project team is very-well qualified to carry out this work and they have a strong record of publication, model-development, and field research in the appropriate areas. The proposal is well-written and well-designed and, if funded, will likely produce high-value results on an issue of critical importance to CBDA.

Technical Review #1

proposal title: Responses of tidal wetlands to invasive cordgrass, sea level rise, and sediment supply

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The goals and hypotheses are clearly stated for the most part. I find that some of the assumptions for the hypotheses (e.g., that literature values of long-term accumulation rate can be taken to use in modeling sediment accumulation), to be naive. The idea is timely and worth exploring, although I feel that the project would do better as a smaller pilot project given that they will only look at two sites anyhow.
Rating	very good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	The knowledge base is extensive upon which this study is based. The questions to be addressed are important and I am surprised that the PIs chose only two sites to look at their variable, given that they seek to constrain the range of potential values applicable to modeling the SF Bay region. I do not believe that this project should be as large as it is. It could be tested in a much smaller way initially. As an aside, the spartina in the SE proagates almost

	exclusively by tiller extension, not by seed dispersal. It will be interesting to see how this difference in growth habit affects the outcome of this study.
Rating	very good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The approach is pedestrian as it is just doing what Morris has done in several other sites again to determine if new parameters are necesary for his model. I am not sure that their assumptions upon which they based their approach is valid, i.e., that the tidal range and turbidity are very different between SF and SC. My tides program tells me that SF Bay has 2 m tides, quite simialr to SC, and the Bay is quite turbid, similar to SC, every time I've flown into the airport there. The PIs do not show any data to support their claim. Also, the SET methods are another bit of technology that has been used alot around the east coast with mixed results. The readability accuracy of the measurements they give (1.3 mm) is pretty much equal to the signal one might expect over the 6 months between readings, making the SET an imperfect tool. The genetics is outside of my field so I cannot comment on the ingenuity of those techniques.
Rating	good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	I have no doubt that the PIs can perform the work and get the data. I only question if the vast amount of data, given that the systems may turn out to be quite similar, is really necessary to collect without some initial trial experiments. The techniques proposed are time-tested and reiable. I do feel that this project is overstaffed given the frequency of sampling once the SETs, organs, etc., are installed.
Rating	very good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	Monitoring is extremely important to this proposal and this aspect has been carefully thought out. Replicates are included and comparative plots are planned. I would have liked to see more than 2 locations sampled however – even if it means forgetting about the sediment geochemistry or SETS. The feldspar markers can give you much of what you need and the extensive geochemistry, although fascinating I'm sure, is time intensive and expensive and probably relates to factors that have much less impact on the productivity and biomass.
Rating	very good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

#0047: Responses of tidal wetlands to invasive cordgrass, sea level rise, and...

Comments	I would look for each PI to give a talk at a meeting or two and one paper to make it out into the literature. Morris has a good record of getting information out into the primary literature, so one paper can be certainly expected. The results from this study could be quite valuable if the hypotheses stated prove correct, in terms of understanding the controls on spartina propagation and productivity. I do wonder why they are making all this fuss, as the hybrids are spreading rapidly and probably can't be eradicated as far as I can tell from the background in the proposal. They will simply document what is going to happen. I was surprised to see the PIs state that their plot scale data cannot be extended to the landscape scale (pg. 7). IF the model results cannot be generalized, then why bother? The results need to be useful on a larger scale than the individual plot for half a million dollars to be invested!
Rating	good

Additional Comments

Comments	I don't understand figure 6C. If you are assuming a extrapolated rate of sea level rise of 0.34 cm/y, and that is what is driving panels a and b, why doesn't the mean sea level line simply go up monotonically at that same rate?

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	All aspects of insfrastructure are available for the project. Although Strong and Morris are the PIs, it is obvious that Ayres will be running this project. Her track record is strong for someone early in their carrer and she seems to publish in a timely manner.

Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	I think that the budget is overlarge for the proposed return. This asssessment would be easier if CALFED used a standard NSF-type budget form, instead of the hourly rate times hours per task as you do. Morris' travel budget is high for the travel anticipated. AS I have mentioned earlier, there are expensive tasks, such as the biogeochemistry, that could be left out or be scaled back to allow more sits to be covered, or done away with to decrese the budget. I also question why they need to put out 12 SETs, given that one SET swings 360 degrees and should cover a large area - and they only will examine two sites. 2-4 SETs is plenty.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	This proposal seeks to answer some fundamental questions and extend the utility of a model for marsh productivity to areas beyond where it was developed and to determine the future of hybrid spartina in the SF Bay. That is laudable. However, I am not sure that their approach is appropriate to the goal and would like to see more sampling sites instead of the intensity of parameters sampled for. Most troubling is the admission that the data they generate from their plots is not extendable to the landscape level - a necessary goal of anything CALFED funds in my mind. The PIs need to keep in mind that spartina in the SE works differently in at

	least a few significant ways from that in the West.
Rating	very good

Technical Review #2

proposal title: Responses of tidal wetlands to invasive cordgrass, sea level rise, and sediment supply

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	This collaborative project between UC Davis and U South Carolina will revise an existing quantitative model for South Carolina marshes to predict the influence of sea level change on salt marsh primary production and elevation change in San Francisco Bay marshes. The model will be revised and expanded to predict process level changes that may occur due to the invasion of native marshes by Spartina alterniflora. Model development will be based on rigorous and ingenious lab and field experiments along with well accepted field survey methods. Project goals are stated clearly with ample supporting detail. The project is extremely timely, given the potential for both the invasive plant and increasing sea level to influence all of CALFEDs tidal marsh restoration projects. The results of this project will be extremely useful in selecting future restoration projects, and adaptively managing existing projects.
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	The project rests on an impressive base of previous work both on the ecological genetics of the native-invasive plant interactions, and on the quantitative response of intertidal vegetated marsh at both small and large scales to changes in sea level. The conceptual and quantitative models of system response to the two drivers of invasive plants and change in sea level are very well explained. This project will provide a powerful framework with which to understand and manage existing and restoring tidal marsh systems in the Bay area. The research described will fill a critical knowledge gap with respect to future tidal marsh dynamics in the region.
Rating	excellent

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The application of the marsh planters and sediment erosion tables to Bay area marshes is very appropriate. This appears to be the first use of marsh planters on the West Coast, and this is possibly true for the SETs as well, although I am aware of plans to install a number of them in Oregon as well. My only methodological question has to do with the influence of herbivory and other plant losses, say through breakage and export, on the in situ measurements of primary productivity. These sorts of losses may be insignificant, but if they aren't, I'm not sure the methods will capture missing production. Do we know anything about the differential susceptibility of the two Spartina species/hybrids to herbivory? The project will produce a very credible model of marsh response to sea level change that will be sensitive to the

	relative abundance of the two Spartina species. One last thought about the planters would be to increase the number of levels in order to bracket the conditions under which the plants can survive. It would be satisfying to show that the plants actually do not survive consistently below and above threshold elevations. I hesitate to make the comment, because I suspect that that Morris has decided that the extra time an effort involved in expanding the planter design is not worthwhile in terms of the model's validity.
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The project participants have established a strong methodological base for their project through years of previous research. This project has a very high likelihood of success, and the scale of the project is clearly well informed by this prior experience. The mix of well replicated field experimentation and survey work in contrasting systems will provide a strong dataset with which to revise model parameters to predict future change in productivity and geomorphology.
Rating	excellent

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	The field survey portions of the study will monitor accretion/erosion, plant productivity, and relative

	abundance of the two Spartinas under two conditions: 10 incipient S. alterniflora hybrid marsh establishment, and 2) conversion of native marsh to S. alterniflora hybrids. These data will provide input for the revision of a pre-existing and successful model of marsh response to changes in sea level. The tight connection between monitoring data and modeling effort in this proposal is to be greatly admired and appreciated!
Rating	excellent

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The project will produce a credible model that will allow predictions of the geomorphologic response of SF bay marshes to the dual threats of sea level change and invasion by S. alterniflora hybrids. The model will be of direct utility to those organizations involved in the management and restoration of SF bay tidal marshes - especially the California Coastal Conservancy, California Fish and Game and the USFWS. These groups will be able to apply the model to determine threatened sites, and to develop plans to manage/adapt to these threats. The outputs of the model are clear and quantitative, and will be of direct applicability to the management community.
Rating	excellent

Additional Comments

Comments	

#0047: Responses of tidal wetlands to invasive cordgrass, sea level rise, and...

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The project participants have impressive track records upon which the project builds in a very efficient and appropriate manner. Both labs are well equipped and experienced in the tasks they will carry out for the project. There is no foreseeable reason why this project should not be highly successful.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget is an appropriate mix of support for PI oversight, post-doc and graduate student support, equipment, supplies, travel and indirect. The indirect costs seem quite reasonable as well. The budget forms do not lend themselves to easy analysis however.
Rating	excellent

Overall

Provide a brief explanation of your summary rating.

Comments	This project excels in every aspect. It addresses very important threats that are not well understood, it outlines a strong theoretical, experimental and quantitative basis for the work proposed, and it describes a mix of novel and well established methods to expand the current understanding of marsh productivity and geomorphologic evolution to account for the influence of sea level change and plant invasion to the vulnerable existing and restoring

	marshes of the Bay area. The model will be of great utility to those who wish to apply quantitative science to the management and restoration of San Francisco Bay's tidal wetlands.
Rating	excellent

Technical Review #3

proposal title: Responses of tidal wetlands to invasive cordgrass, sea level rise, and sediment supply

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The project goals are focused and clearly stated: to understand how hybrid cordgrass invasion, sediment supply, and sea level rise affect long-term sustainability of salt marshes in the San Francisco estuary. These goals are timely given the rapid spread of Spartina alterniflora and its hybrids with Spartina foliosa in the Bay and the threat of sea level rise to coastal wetlands. The goals are directly relevant to CALFED priorities of managing and restoring habitats and are of current interest in the scientific literature.
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	The project is well-justified by the current risks of invasive cordgrass and sea level rise, and takes an approach similar to that used effectively by one of the PIs in South Carolina Spartina marshes. The conceptual models (pp. 9 and 18) are founded on current knowledge about cordgrass hybridization and spread, and how salt marshes respond to sea level

	rise, sedimentation, and plant productivity. The proposed research directly addresses the interactions presented in the conceptual models.
Rating	excellent

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The approach focuses on developing parameters for a model to predict change in the elevation of the surface of salt marshes as a function of aboveground biomass, depth below MHW, sediment loading, and sediment trapping ability of vegetation. Elevation and accretion will be measured using the SET, a state of the art technique for measuring parameters affecting wetland elevation changes, while "marsh organ" planters will be used to study biomass-depth relationships. Primary productivity and biochemical parameters will be measured using standard techniques. These aspects of the project have been applied successfully in South Carolina marshes. The unique application to the San Francisco marshes is its focus on how marshes containing native Spartina foliosa, invasive non-native S. alterniflora, and their hybrids respond to sedimentation and sea level rise. The research group of one of the PIs has studied hybrid cordgrass genetics, an understanding of which is essential for this project. While I have little doubt that the PIs have the experience and knowledge to rigorously implement the experiments, the methods section (pp. 11-15) is not particularly well-written. For example, it is unclear how many SETs will be installed per site. Also, the number of marsh planters per site and the number of levels in each planter are not clearly stated; on p. 14 it says there are 8 planters, while on p. 11 it says there will be 4

	planters for each of 4 genotypes (16 planters?). Finally, there is mention of control and fertilized plots in the SET methods, which I assume were copied from another proposal or report erroneously. Finally, the interpretive analysis section (p. 15) is interesting but is only for SC marshes and is not tied in with the proposed research in the San Fransisco marshes.
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The research proposed is feasible and has a high likelihood of success. One of the PIs has used many of the techniques in SC marshes, and the other has studied cordgrass hybrids in the San Francisco estuary.
Rating	excellent

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	Monitoring is involved in this project, but its goal is not to compare restored vs reference sites. The proposed monitoring approach is scientifically valid.
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

#0047: Responses of tidal wetlands to invasive cordgrass, sea level rise, and...

Comments	The major product from this research will be the model that predicts marsh elevation based on sea level rise and sedimentation for San Francisco estuary marshes dominated by S. foliosa, S. alterniflora, or their hybrids. As such the model will be very useful to environmental managers for managing or restoring salt marshes in the estuary. Other products include peer-reviewed publications, presentations at a CALFED conference, a CA weed control conference, and a national scientific meeting.
Rating	excellent

Additional Comments

Comments	This is an elegant study: the techniques used are simple and should provide clear, important results.

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The project team has outstanding capabilities for performing this work. Strong and Ayres have extensive experience with hybrid cordgrass in the Bay, and Morris have used the experimental, monitoring, and model development methods successfully for SC marshes.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget seems quite reasonable given the amount of work proposed and the potential significance of findings from this project.
Rating	

	`excellent`

Overall

Provide a brief explanation of your summary rating.

Comments	`This is an interesting and unique approach that addresses the important issue of salt marsh elevation in the Bay under conditions of rising sea level, variable sediment supply, and invasion by hybrid cordgrass. The work is sound science that should yield a model that will be valuable to environmental managers.`
Rating	`excellent`